inflammation.[1] In two of these cases a haemolytic streptococus was recovered as the presumptive causal organism. In one of these patients, as in the cases reported by Dr. Statham and Dr. Morton, there was a more rapid relief of the signs and symptoms of the pelvic inflammation once the device was removed than with antibiotics alone.

While it is generally agreed that the occasional case of severe pelvic infection can occur in association with an I.U.C.D.[2 3] it is with the lesser degrees of pelvic inflammation that there is a difference of opinion. We have recently pointed out[4] that there is a significantly higher incidence of symptoms and signs which could be attributed to pelvic inflammation in women who were using an I.U.C.D. with a cervical appendage (11–21%) than in those using an I.U.C.D. which was totally intrauterine (2%). In our experience the pelvic inflammation responds to repeated courses of broad-spectrum antibiotic therapy occasionally reinforced with pelvic shortwave diathermy. It is only in the more severe cases that it is necessary to remove the I.U.C.D.

In order to assess the sequelae and significance of this diagnosis of pelvic inflammation, a controlled salpingographic study of 50 cases was instituted following treatment for pelvic inflammation. Within this series were seven cases of bilateral tubal occlusion and 16 cases of unilateral occlusion. Localized tubal damage appeared to be present in a further 15 cases.[5]

In view of this we would like to emphasize that the I.U.C.D.s that are currently available, most of which have a cervical appendage, should not be used in nulliparous women, or in women who wish to be sure of retaining their fertility potential.—We are, etc.,

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REFERENCES

1 Elstein, M., *Proc. roy. Soc. Med.*, 1967, **60**, 397.
2 *Wld Hlth Org. tech. Rep. Ser.*, No. 332, 1966.
3 Tietze, C., *Amer. J. Obstet. Gynec.*, 1966, **96**, 1043.
4 I.P.P.F. Medical Bulletin, 1968, **2**, No. 3, p. 3.
5 Elstein, M., *Int. J. Fertil*, 1969, in press.

Tetracycline and Nystatin

SIR,—The recent report by the Clinical Trials subcommittee of the British Tuberculosis Association on a multicentre, double-blind, controlled trial comparing the side-effects of tetracycline and tetracycline plus nystatin (16 November, p. 411) concludes that their findings "failed to show any association between candida and gastro-intestinal symptoms normally attributed to chemotherapy," and that "the addition of nystatin to tetracycline cannot be justified at present on the grounds that it suppresses the upgrowth of *Candida* in the bowel and hence symptoms caused by *Candida*." I submit that the methods used during this trial could not possibly allow the authors to arrive at any valid conclusions.

The 143 patients admitted to the trial suffered from sufficiently severe chest infections to be admitted to one of the eight trial centres. Most of them were suffering from severe chronic bronchitis and had many previous courses of oral broadspectrum antibiotics. At least 36 were already ingesting antibiotics on admission to the trial and at least 70 had already alimentary tract symptoms before the trial was started. It seems more than likely that the vast majority of these patients had florid oral candidosis, particularly if they wore dentures. This site is a common source of recontamination of the gut even during treatment with nystatin tablets, as the latter neither act on the mouth nor are they absorbed.

If the aim of this trial was to establish whether the alimentary tract side-effects of oral tetracycline were related to the concomitant increase of faecal *Candida* and whether these side-effects could be prevented by giving tetracycline combined with nystatin (Mysteclin), in my opinion it is essential to clear all patients so far as possible of any excess of *Candida*, and that only after such preparation would it be fair and meaningful to commence a double-blind trial.

While rectal swabs are certainly the easiest way of sampling faecal flora, they are consistently positive for *Candida* only if the fungus is present in the gut in large numbers. For the purpose of a trial of this kind it would have been important to have *repeated stool cultures*, preferably daily, starting several days before commencement of the trial, and continuing for at least one week after medication has finished, as some patients develop abdominal side-effects only at that stage. It is, of course, essential to culture at the same time scrapings from the corners of the mouth, the mucous membrane over the hard palate in denture wearers, the commonest site of chronic asymptomatic candidosis, the perineum as well as vaginal swabs. It seems regrettable that no attempt was made to assess the response of patients to *Candida* by immunological techniques. There cannot be any doubt that the majority of patients on oral broadspectrum antibiotics with a demonstrable increase of *Candida* in their stools do not show any side-effects; however, those who do may have frank candidosis of the bowel mucosa or show marked allergy to the yeast on further investigation.[1 2] Most reactions to *Candida* can be measured by a number of established immunological techniques.

The information obtained by the clinical investigators with the questionary on symptomatology must be of doubtful value. Apparently patients were merely asked whether they had a sore mouth without any effort being made to examine the oral cavity after dentures had been removed.

Flatulent distension, frequency, and volume of bowel motions are all measurable and should have been objectively assessed. Many patients admit to pruritus ani only when told that the perianal skin looks macerated and scratched. The term "skin rash" is meaningless unless its characteristics are defined. A differently arranged, and admittedly more complex trial, may yield much-needed information on this important subject.—I am, etc.,

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REFERENCES

1 Holti, G., in *Symposium on Candida Infections*, edited by H. I. Winner and R. Hurley, 1966. Edinburgh and London.
2 Holti, G., *Brit. med. J.*, 1967, **1**, 155

Epidemic Influenza and General Hospitals

SIR,—May I be permitted to make two comments concerning your leading article of 14 December (p. 655)? Both concern the failure to mention *Haemophilus influenzae* in it. While it is true that in recent epidemics this organism has not posed the problems that it did in the pandemic of 1918–19, the possibility that it might again become prominent should certainly not be overlooked. Further, influenza sufferers who also have chronic bronchitis will almost certainly develop an acute haemophilus broncho-bronchiolitis, which in such circumstances could be as dangerous as staphylococcal pneumonia.

For these reasons I suggest that routine chemotherapy should always include an antibiotic active against *H. influenzae*. Your suggested regimen of benzylpenicillin and cloxacillin would have no effect upon this organism, and in my opinion ampicillin 250 mg. intramuscularly six-hourly should be substituted for the benzylpenicillin.—I am, etc.,

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Asian Flu Vaccine

SIR,—I have not yet been able to obtain any Asian flu vaccine for any of my chest or heart patients. I hear that the staff of a local television rental business have been vaccinated.—I am, etc.,

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Pain in the Face

SIR,—Working in a mysterious way against themselves, Messrs. P. R. R. Clarke and J. Hankinson (2 November, p. 328) have taken Olivecrona's[1] operative mortality of 2 in 445, or 0.45%, and have doubled it. As if to restore the balance, they have also awarded Rowbotham[2] "250 cases of trigeminal root section with no mortality" instead of the 132 which he claimed. Frazier[3] reported 156 consecutive non-fatal cases of this operation, Horrax and Poppen[4] 176, and Cushing[5] 312. As an inspiration, series such as these are valuable; statistically, each of them is worth no more than its opposite, the series of the same length with most deaths. The belief that they constitute information about the death rate might be termed the consecutivist fallacy. But why depend on finite numbers at all, when you can quote Leriche[6]? "*Il n'est plus question pour elle* [that is, for trigeminal rhizotomy] *de mortalité opératoire.*"

To come nearer to earth, what has been the death rate in any consecutive series of at least 500 subtemporal trigeminal rhizotomies in all hands at a good surgical centre? This, being the rate for all patients operated on at such a centre, deserves to be called the true death rate. The answer was 1.9% in one series[7] of 1,106 cases, and 1.6% in another[8] of 553.

The recurrence rate after complete rhizotomy is probably about one-quarter of that after partial rhizotomy. Any alleged series